the contents of the young sieve-tubes. Isolated drops or irregular masses appear in the layer of protoplasm lining the cell cavity before the disappearance of the nucleus. These consist of a slimy stuff (*Schleim*) apparently rich in nitrogen.[1] The separate masses later fuse together to form a band, which is usually much narrower than the girth of the cell. Between this and the wall of the sieve tube a protoplasmic envelope intervenes (*Hüllschlauch*). The central cavity within these is filled with "sieve-tube sap." For further details concerning the contents of the sieve-tubes the reader must be referred to the original work.

The author has not been able to observe directly the first appearance of connection through the pores of the sieve; but suggests that it is effected by the outgrowth of protuberances of the envelope (*Hüllschlauch*) from opposite sides of the sieve, which penetrate it and coalesce to form the connecting strings.

The presence of starch grains noticed by Briosi is confirmed by Wilhelm in Vitis. He finds them in members of sieve-tubes which are still closed. He opposes the idea that they pass through the sieve on ground of their size. In Cucurbita and Lagenaria they are absent. Besides the communication of sieve-tubes with one another laterally, so as to form a complete system, Dr. Wilhelm has observed in the case of Vitis a further connection, through the medullary rays, of tubes lying on opposite sides of the ray. This is effected by special sieve tubes, produced by transformation of cells of the medullary ray, so as to form a series of very short members; these correspond in development and structure with the ordinary sieve-tube. They traverse the medullary rays in an obliquely tangential direction. Such communications are not found in Cucurbita or Lagenaria.

The question of function has not been solved by these observations. Dr. Wilhelm still holds the view, propounded by Nägeli, that the function of the sieve-tube is the transference of indiffusible substances from place to place in the plant.

In conclusion it may be remarked that the paper is well written, but that it is of such a character as to be interesting only to the specialist. The plates, of which there are nine, are executed with great skill and exactitude.

F. ORPEN BOWER

OUR BOOK SHELF

The Elementary Geometry of Conics. By C. Taylor, M.A. Third Edition. (Cambridge: Deighton, 1880.)

MR. TAYLOR has been before the public as a writer on geometrical conics since 1863, in which year he brought out his "Geometrical Conics"; in 1872 we have the first edition, and in 1873 the second edition of his "The Geometry of Conics," a smaller work than his first book (1863). Now we have a third edition with the above title. In May, 1875, Mr. Taylor, in a paper entitled "On the Method of Reversion applied to the Transformation of Angles" (read before the Mathematical Society, and subsequently printed in a more extended form in the *Quarterly Journal*, No. 53, 1875, with the title "The Homographic Transformation of Angles"), called attention to a "neglected work on conics by G. Walker, F.R.S. (1794)": in this work we first meet with the properties of a circle, which Walker calls the *generating circle*, but which Mr. Taylor, in the work before us, styles the *eccentric circle*; in the free use of this circle consists the main feature in the alterations made in this new edition; further, though still keeping well in view the proving chord-properties independently of tangent-properties, there is a rearrangement of the text; so that the two properties are not treated of in distinct chapters. In other ways also we think this little work is improved, but we need say no more upon a third edition.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Ceraski's New Variable Star

UNLESS the principal fact mentioned below has already come to your notice, you may like to bring it before the astronomical public in the columns of NATURE.

The true period of the variable star recently discovered at Moscow (*Durchmusterung*, zone + 81°, No. 25) appears to be two days and a half, instead of five as given in NATURE, vol. xxii. p. 455. Minima were observed at the Harvard College Observatory on September 23 and 28. The changes of the star will accordingly be visible in England on October 13, 18, 23, 28, &c., during the three or four hours before or after midnight. The rapidity of the change is probably greater in the case of this star than in that of any other known variable, the variation exceeding a magnitude in the course of one hour. The total variation is more than two magnitudes. A star of about the eighth magnitude (No. 30 of the same zone) is within a few minutes of the variable, and may readily be compared with it. The phenomenon of the variation is consequently a striking one, even as seen in a small telescope. The approximate place of the variable for 1881 is in R.A. oh. 51m. 48s., Decl. +81° 14′·1.

EDMUND C. PICKERING

Harvard College Observatory, Cambridge, U.S., October 2

LORD LINDSAY'S Dun Echt Circular, No. 10, which I received on Saturday morning, October 23, prepared me to watch for a probable minimum of M. Ceraski's remarkable variable star B.D. + 81° 25′ on the same night. From my observations the minimum appears to have occurred at about 11h. 10m. G.M.T., the star then being of about 9·1 magnitude. At 9h. 5m. I noted it about equal to a neighbouring star, B.D. + 81° 30′, which I gauged 8·1 mag., and at 13h. 50m. it had regained the same magnitude. When about minimum I thought the variable to be slightly ruddy, but as it brightened up again it lost this tint and appeared to be white, or bluish white, as when I first observed it. It has a small bluish 11½ mag. companion, the P. and D. of which I roughly estimated to be 60° and 10″ respectively. The star was observed by Carrington in 1855, on December 19, 21, and 30, his estimated mags. being 8·0, 8·0, 9·0. Possibly the star may have been near minimum at his third epoch.

Knowles Lodge, Cuckfield, October 25 GEORGE KNOTT

"Solid Ice at High Temperatures"

THE interesting results announced by Prof. Thomas Carnelley, of Firth College, Sheffield, in relation to the physical conditions under which *ice* persistently maintains its *solid state* when exposed to the influence of heat (NATURE, vol. xxii. p. 435), deserves some notice. When he speaks of obtaining "solid ice at temperatures so high that it was impossible to touch it without *burning* one's self," it is evident that this *burning quality appertains to the hot vessel containing the ice, and not to the solid ice itself.* For it is obvious that under the given conditions *the temperature of the surface of the ice is kept at least as low as* 0° C. *by the rapid vaporisation of it while in a solid state.*

The phenomenon of a body remaining persistently at a low temperature when surrounded by a hot vessel—through the influence of the rapid change of state—is analogous to the well-known results of Boutigny and Faraday in relation to the freezing of water and mercury in a hot vessel by means of large

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[1] Cf. De Bary, "Vergl. Anat.," p. 185.

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globules of sulphurous acid and liquid carbonic acid while in a "spheroidal condition." In these cases, notwithstanding the proximity of the hot vessel, the temperatures of the globules of SO_2 and CO_2 are respectively as low as – 10° and – 73° C.

It has long been remarked by physicists that some substances pass *directly* from the *solid* to the *gaseous* state, without undergoing liquefaction: that is, when heated, *they sublime without melting*. Such bodies, under ordinary atmospheric pressures, have their *boiling* points *lower* than their temperatures of *fusion*; hence they *volatilise* without *melting*. Moreover it has long been known that such substances may be made to *fuse* by subjecting them to an *abnormal pressure* sufficient to *raise* their boiling points *above* their points of fusion. Thus the classical experiments of Sir James Hall show that carbonate of lime may be *fused* when heated under a pressure sufficient to prevent the CO_2 from escaping (*Trans.* Royal Soc. Edin., vol. vi. pp. 71–186, 1805). In like manner metallic arsenic sublimes without melting at 180° C., under the ordinary pressure of the atmosphere; but the experiments of Landolt in 1859 show that under artificial pressure it melts in globules at a low red heat. It is evident that in these cases the *rapid vaporisation* of the solids under ordinary circumstances *prevents* the temperature from reaching the point of fusion; but when subjected to additional pressure the conditions of liquefaction are secured. On the other hand, in the case of *ice*, it is obvious that the *withdrawal of pressure* by *lowering* its boiling-point places it in the same category with metallic arsenic under ordinary conditions of pressure.

In relation to the *literature* of this subject it is proper to add the following quotations from M. V. Regnault's "Elements of Chemistry" (American Translation, Philadadelphia, 1865, vol. i. p.279). In speaking of the fusion of metallic arsenic under pressure he says:—"The distance between the point of fusion and that of ebullition of any body may, however, be increased at pleasure. For *the point of ebullition of a body is the temperature at which the tension of its vapour is equal to the pressure exerted upon it*; and hence by increasing the pressure the boiling-point is raised without sensibly affecting the point of fusion." Again, he says:—"Reciprocally it is evident that a *volatile solid body* may be always subjected to so slight a pressure that it will *boil* at a temperature *inferior* to that at which it *melts*. Thus ice at the temperature of – 1° C. possesses an elastic force represented by 4·27 mm.; in other words, it boils at a temperature of – 1° C. under the pressure of 4·27 mm. Ice may therefore be entirely volatilised *by ebullition* under this feeble pressure, without reaching its point of fusion, which is 0° C."

Berkeley, California, September 30 JOHN LECONTE

Wire Torsion

THE phenomena described by Major Herschel in his letter to NATURE, vol. xxii. p. 557, and about which he asks for information, are, we think, quite easily explained by what is known of the fluidity of metals. Yielding, or flowing, seems to occur in all metals after a certain limiting stress has been reached; indeed it probably occurs, although perhaps to an immeasurably small extent, even with small stresses (see *Proc.* Roy. Soc. No. 204, p. 411, 1880); but there is generally a limiting stress beyond which the increase of strain due to yielding becomes comparable in magnitude with the ordinary strains, which instantaneously disappear on the removal of the load. The bell-smith pulls his copper wire, and makes it much longer before he thinks it fit for use; in a similar way the telegraph constructor stretches, or *kills* the iron wire before he erects the line. Up to a certain limit of pulling force, the wire obeys the well-known laws of elasticity; slightly above that limit there is considerable fluid-yielding, there being but very little yielding below that limit; and at any instant during the lengthening if the man ceases to pull, the wire shortens a little. In fact at any stage the wire obeys the elastic law for *small* stresses. Eventually the man ceases to pull, knowing that the metal has lost most of its fluid properties, which can only be restored to it by annealing. The same thing occurs in brass, although to a smaller extent than in copper, which can be experimentally proved in the following way:—Stretch a piece of well-annealed brass wire in the manner described by Major Herschel until it is nearly breaking; and immediately set the wire vibrating. Now the note given out by the stretched brass wire, which, as is well known, depends on the tensile stress, will be found rapidly to go down in pitch. If the wire be tightened up again sufficiently with the screw, the original note will again be heard, and the pitch will again go down, but not so rapidly as before. Repeat this process until no flattening of the note is heard; then in this state we think that the experimenter will find the wire to break with much less torsion than before, and to obey Hooke's law more exactly. If it be desired to repeat the yielding or flowing process, the wire must be previously again annealed.

Mere sudden straining, even nearly up to the breaking stress, is not sufficient to destroy the fluidity of brass; time is required. The yielding behaviour of a brass beam when loaded has been studied by Prof. Thurston (*Trans.* American Soc. of Civil Eng. vol. vi. p. 28), and we may add that we have found that the permanent state is always more rapidly reached when the wire is subjected to rapid vibrations.

It may be because torsion of a wire is more visible than longitudinal strains (the twist being inversely proportional to the fourth power of the diameter for a given twisting moment, whereas the longitudinal strain for a given load is inversely proportional to the square of the diameter) that fluidity is so much more apparent in torsional experiments; but we think it probable that fluidity will be found always much more apparent when the volume of the material acted on is unchanged, that is, when the stress is mainly one of shear as it is in torsion.

However this may be we can explain why wire which has been "killed" for pulling forces is not "killed" for twisting, and why it is more difficult to kill for twisting than for tensile stresses. It is well known to wire-drawers that in whatever state copper or brass wire may be, whether annealed or not, it may be drawn smaller, although no doubt it requires less care to draw it if it is annealed. We cannot merely pull wire much smaller, it has to undergo a lateral pressure such as the die gives it. Now in twisting a wire it everywhere receives this lateral pressure, that is—imagine a right-handed spiral filament being lengthened by the twist—then the other component of the twist gives to the filament a compression at right angles to its length which enables it to extend. It seems that this lateral pressure is needed to overcome some sort of friction in the particles of the metal tending to prevent their moving into the axis of the wire, and which therefore is greater as the section of the wire is larger, and it is probably for this reason that a very thin wire extends much more, for a given initial length, before it is killed than a thick wire. We have known a length of about fifteen inches of fine copper wire which had just been drawn, and which had been well killed, to bear six or seven hundred complete turns in a lathe, one end being fixed, the other end turned, and the wire kept pretty taut before it was accidentally broken, and even afterwards parts of the wire could be considerably lengthened by pulling. The nature of the explanation of this apparent annealing for tensile stresses arising from previous torsion will be gathered from what follows.

We infer that the three or four turns given to the wire at the beginning in Major Herschel's experiment were not sufficient to produce permanent torsional set; why then should increasing the tension during the torsion cause torsional set as well as lengthening of the wire? This is, we think, a more important question than the one presented to us by the observations of fluidity in the latter half of Major Herschel's letter, and which arose from the metal having belonged to what Prof. Thurston calls the "tin class" as distinguished from metals of the "iron class."

The explanation we think is as follows, and it leads to the conclusion that torsional fluidity is not independent of tensile stress:—

Suppose right- and left-handed spirals had been imagined in the wire in question, making everywhere angles of 45° with the axis of the wire; then torsional strain, however set up, would consist in the production of a difference in length of these two sets of spirals. Now a twisting moment produces this effect; it lengthens, say the right-hand spiral and shortens the left, and we know that up to a certain limit, which is tolerably high, the same effect is produced whatever be the tensile stress in the wire, which latter simply tends to lengthen both spirals equally. In fact if Hooke's law is true, the torsion is independent of the tension. But above a certain limit of pull in the wire, the strain in the direction of the right-handed spiral being everywhere due to the sum of two tensile stresses, becomes so great that fluidity sets in and permanent set is produced; whereas in the direction of the left-handed spiral the stress is due to the difference between the tensile stress and the compressive part of the torsional shearing stress, and this difference being small, no permanent tensile set is produced, or at all events one much less than in the case of the other spiral. Consequently if all stresses now cease to act

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